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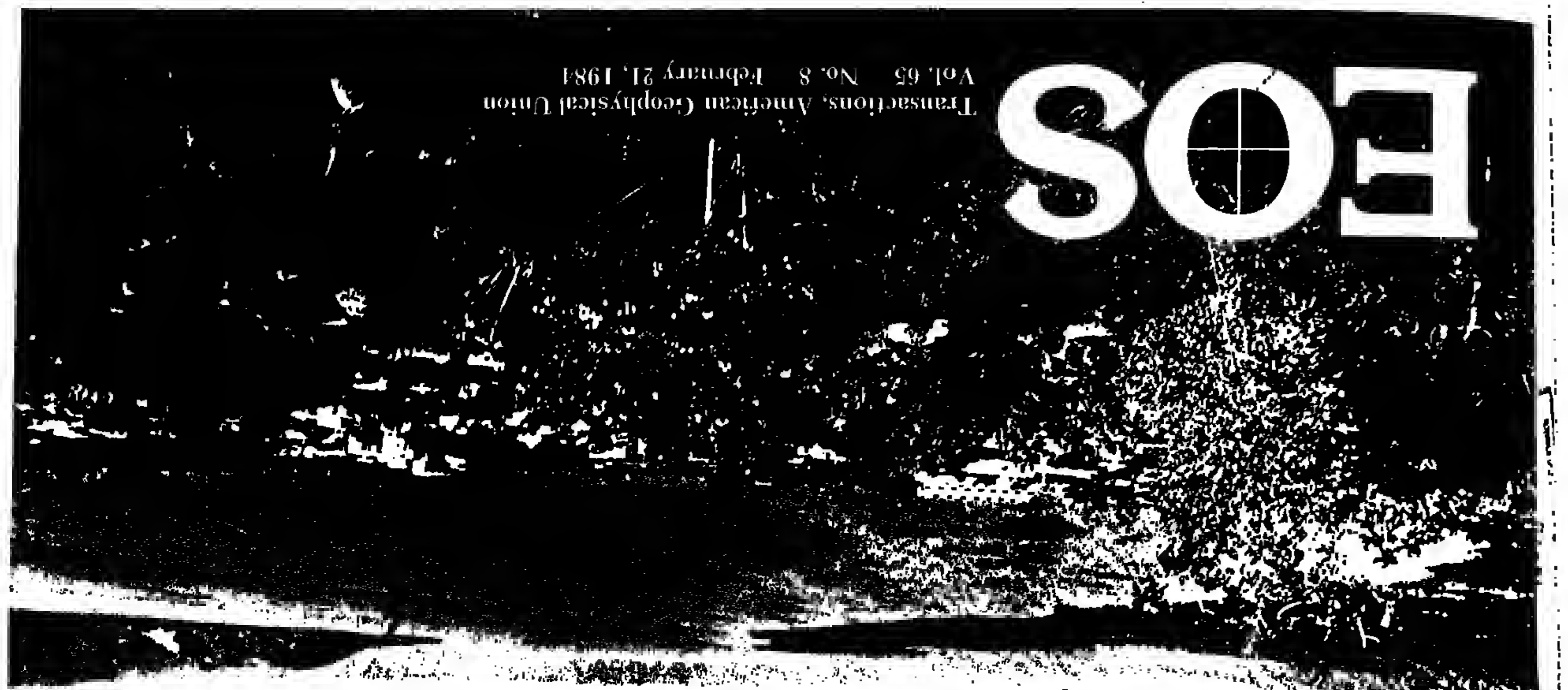

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Seismology

Tectonophysics

Volcanology

E. C. Bullard's First Heat-Probe

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Sir Edward Bullard in 1973 said to me, "Betty, I want to tell you how my first ocean heat-flow instrument was made in the summer of 1949." He was then on his annual 3-month visit to Scripps Institution of Oceanography and knew that I was writing a history of the institution. Teddy (as he preferred to be called) was proud of his participation in measuring the flow of heat at sea. The instrument that he described that day never made a heat-flow measurement, but it did establish the technique that has been used for more than 30 years.

The following account is a slightly shortened version of our taped conversation on August 5, 1973 (the original audio cassette is in the archives of Scripps Institution of Oceanography, item 81–84). Unfortunately, the reader loses the charm of Teddy's voice and accent, but here for history are his words.

Bullard: I first became interested in heat flow in the early 1930's. I'll send you a copy of a paper that describes some of the background of the early work on land [*Bullard*, 1939]. It was the British Association, which is like your triple-A S—had a committee on the subject, with Harold Jeffreys on it. In the period just before the war I was developing methods of making heat-flow measurements on land, which had been done extraordinarily badly before, and hardly anything had been done since about 1880. There was a previous British Association committee way back in the 1880's, and I have written a historical paper on this. The historical paper is in Willie Lee's book on heat flow [*Bullard*, 1965]. He'd asked me to write a summarizing chapter for the book, and I said I wouldn't do that, because it's too dull a thing to do, and then he wrote to me and said he thought this was most unreasonable and improper of me, and he saw no reason why I shouldn't write a summarizing chapter. And so I wrote an historical chapter which was really a sort of joke chapter and was mostly about the study of heat flow in the 17th century. But it does give the more recent stuff.

Well, then, shortly before the war I began to think about measuring heat flow at sea. And the war in '39—the war overtook the plan, and we didn't do anything about it.

Shor: Had you developed an instrument at that time?

Bullard: No, I had developed the ideas for the instrument but we had done no hardware development, but I knew what I wanted to do. And we had two schemes for doing it: one was the one we eventually adopted of putting a spike in, and the other was the idea of putting a kind of mattress on the seafloor and leaving it there and measuring the temperature gradient through it. Then, after the war I couldn't do this because I couldn't get a ship. *Discovery* was being used for replenishing lighthouses, and there was no British oceanographic ship available immediately after the war. Then I tried to get Maurice Ewing to cooperate. He said he couldn't spare any ship time for this purpose. He'd just gone to Lamont. I'd known him—I'd been to sea with him before the war when he was at Lehigh, in connection with seismic work.

Well, then I went to Canada. I took a job in Canada, going up there in March 1948. And in the summer of '48 I paid a brief visit to California. It was my first visit to La Jolla. Louis Slichter used to run a conference on geophysics every year [at the University of California, Los Angeles]. I went to that, and Louis brought me down here afterwards, and I came down here and met Roger Revelle and Walter Munk. My memory is that I went to a party at Roger's, and I met a lot of blondes. I remember La Jolla as being full of blondes.

I got on very well with Walter and Roger. They asked me to come back the next summer. I forget exactly when, but I got a letter inviting me to work for the summer down here, and I said there were several things I'd like to do and one of them was the heat-flow measurement. And I came down here, probably June, July 1949. And I got here and it was suggested that I take on Art Maxwell, who was a graduate student.

Elizabeth N. Shor was raised in Lead, South Dakota, attended Pasadena City College (1 year) and Wellesley College (2 years). She has learned a bit about geophysics from George G. Shor, Jr., whom she married in 1950. She has written two books on the history of paleontology, one on the history of Scripps Institution of Oceanography (where George works and she has worked), and a number of biographical accounts of scientists, especially for the Dictionary of Scientific Biography.

Well, anyway, I decided to do two things: to investigate the westward drift of the earth's magnetic field, which was a data analysis problem, and to try and build an apparatus for measuring heat flow. Well, then I went to Roger and said we'd like some workshop time. And Roger said he was sorry, the workshop was completely taken up with urgent work, and it was impossible to give any workshop assistance. So I decided that we'd build the thing ourselves. So I went around to the fellows and I hadn't got any workshop assistance—couldn't we work in the workshop, Art Maxwell and myself? This was obviously a slightly embarrassing matter. But we went down to the old workshop in the old shed. I got an office—I shared an office with Walter Munk, over the old director's office in the original building of Scripps.

Well, we settled down. This was a substantial job. We designed what—it was the first time I think that anyone at Scripps had had a watertight equipment for use in the deep sea. I remember the expert advice from [John D.] Isaacs, and so on, was very much against this. They wanted us to build an equipment that was full of oil — the recorder, the electronics and everything immersed in oil. And I decided against this. I decided to build a watertight apparatus made watertight with O-rings. That was, I think, brand new. And we had some interesting experiences with it.

First of all, we had great difficulty in getting sufficiently strong tubing—alloy steel tubing seemed not to be available in sort of 1-inch thicknesses 6 inches diameter. We eventually found that the people who made ball races—ball bearings—made them by slicing them off steel tubes, and one of the manufacturers of ball bearings and ball races supplied us with a piece of tubing. And we got some end plates out of good alloy. The stuff was about 6 inches internal diameter and about three quarters of an inch thick, something like that. And we made up things out of this, with O-rings. Of course, the O-ring had been introduced originally for aircraft-hydraulic systems during the war and was then introduced into the uranium diffusion plant as a matter of making it pressure tight. And I'd known of it from these sources. And we decided to use this. And it proved extremely effective. We went out and tested these containers, and we never had a leak. I've never had a leak from an O-ring container at sea. They're used in everything now of course. They're used in cameras and seismic equipment and all kinds of things.

And we made a very clumsy apparatus for this purpose. We decided to do something which I thought I understood rather than something I didn't. So we bought—at least I got from the British Admiralty—galvanometers, which were very robust and which could be used horizontally. And we fixed these and used photographic recording and thermocouples. So it was thoroughly old-fashioned equipment: no electronics; photographic recording, galvanometers, reversing switches for eliminating thermal emf's in the equipment. And this equipment I afterwards used myself, so it is the equipment I describe in my first paper [*Bullard*, 1954].

Well, we decided to allow the full pressure to come on the probe itself. So the probe itself was filled with oil, and the recorder was full of air. And we had pressure-tight connections between the two, and—sort of looking back—the whole technique of fitting the connections through—I remember we had a good deal of trouble with getting the connections through, and eventually I decided there was too much advice in the workshop on this, and I took the end plates home. I took this home, and heated it on a gas range and soldered the connections in—little glass connecturs with a little metal skirt, and soldered them in.

Shor: Was Art Maxwell working closely on this?

Bullard: He was working on this—we were both working on the lathe making this thing. Of course, we had to make this photographic recorder; you see, we had to make a photographic camera and all the associated supports and things. Well, anyway, we made this apparatus in the course of about a couple of months. And we took it to sea. We had trouble in getting ship time. What we wanted was a day or two of ship time to go out and test it, you see. We did all the testing. I remember the first testing we did, we built a sort of gallows down at Mission Bay [in San Diego], which was then a marsh, of course—and strung the apparatus up with the galvanometer and things in it and climbed up a ladder and cut the ropes and let it drop to see if it would go into the mud and see whether the shock would break the galvanometer and all this kind of thing. Anyway, we then tried without any galvanometers in to see if we could get it into the mud out in the trough off San Diego. There was a tremendous amount of sort of testing to be done. It was an entirely new thing really.

We discovered the trouble that if you put a thing into the bottom and left it there you couldn't avoid pulling it and bending it, and we tried tubes of various lengths and various diameters, to see what we could do. And we more or less discovered what we could do and what we couldn't do. And finally we took it out for a run with the whole apparatus to try and measure the heat flow—and we got no records at all. Everything worked. Except we didn't get any records. And we spotted what it was. It was condensation on the mirrors of the galvanometer, which we'd not thought of before. So we put drying equipment in and got rid of this. But we never got any results that summer, and I went home [to England]. I was director of the National Physical Laboratory [NPL]. I went home and had another similar apparatus built, and I got results with that.

That was built of course by the NPL workshop. We had some of the best workshop facilities in the world. I was at home. I had superb facilities and no question as to who was going to use them. You see, I was director of an operation spending many millions of dollars a year. If I said do something, they did it. And so I had proper working drawings made of the equipment and had it built in a proper workshop and so on. And we took it out and it worked every time.

Shor: You tested it then off England?

Bullard: Yes, I'd done all the testing here so I just made a new apparatus and took it out to sea and it worked.

Now that's all been published, so the dates on this are all known. I'll give you all the references. Our first paper was published in the *Proceedings of the Royal Society*. You know the two letters in *Nature*, do you?

Shor: No.

Bullard: Well, I'd better find these for you [*Revelle and Maxwell*, 1952; *Bullard*, 1952]. What happened was this. There was a little—well, not exactly a misunderstanding—but I understood that the group here and ourselves were going to publish a joint paper. And I was very busy and I held up publication of our work for some time, and eventually the [Revelle and] Maxwell letter to *Nature* went in, and I put a letter in in the same number. Well, then afterwards we published a joint paper [*Bullard et al.*, 1956]. There was a bit of a muddle about publication.

Do you know—now I come to think of it, the business about taking the thing home to make the go-through connection was at NPL; it was the NPL workshop that was making a scene about go-through connections—pressure-tight connections in heavy plates. And I took it home and did it on my domestic gas range. And I think this rather shook the place, to have the director take home the thing and actually do the soldering when they said they thought they couldn't do it. That was in England. It wasn't here. We did it here without any fuss. We did it ourselves in the workshop.

The workshop manager was very good to us and very nice to us. And eventually, actually, some of the heavy machining was done. I said to him one day, I said, "Look, do you want us doing the heavy machining? These are big lathes. We're not professional mechanics. We're turning considerable material, these alloy steels. Wouldn't you rather do this yourself?" And they did—the actual outer container was machined by the workshop staff in the end. But we machined the rest of the stuff.

Now there is a complete description—the apparatus we had here is essentially the same as the apparatus I built in England.

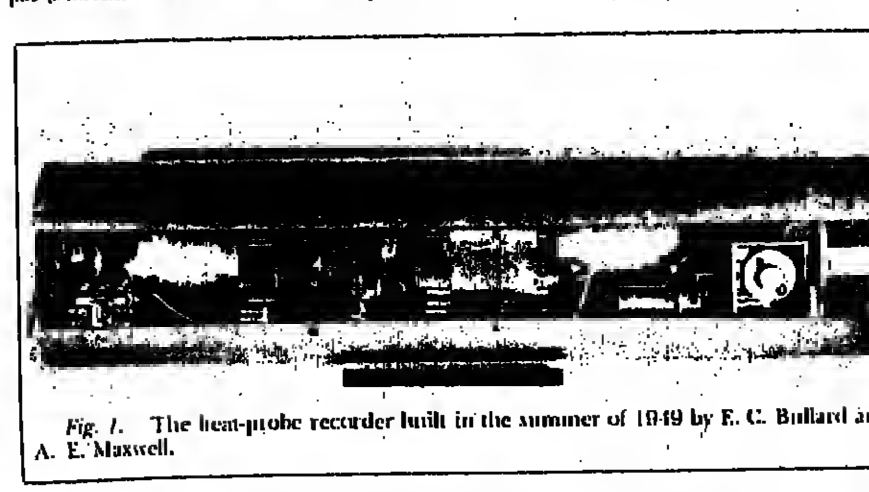
So that's essentially the story. And then afterwards Art Maxwell, Roger [Revelle], and I wrote a summarizing article on the whole subject. It's a review article about heat flow. The critical things to look at are those two letters in *Nature*, which are the first publications, then there is my paper in the *Proceedings of the Royal Society* describing my own measurements, and there's our joint paper with Roger.

So ended Bullard's verbal summary in 1973.

After the unsuccessful trials of 1949, the original instrument at Scripps Institution was "discarded in favor of using thermistors in a bridge network with an amplifier and home-built recorder," said Arthur E. Maxwell (personal communication, June 21, 1983). He worked with Scripps engineers James M. Snodgrass and John D. Isaacs on the improved model during late 1949 and early 1950. That heat-probe was used on the Midpac (sometimes called Mid-Pacific) Expedition, carried out by the Scripps Institution of Oceanography and the U.S. Naval Electronics Laboratory during the summer of 1950. (The name of this expedition is given incorrectly in *Bullard* [1963] as Capricorn Expedition, which was in 1952.) The first successful oceanic heat-flow measurements were taken on that expedition. (As noted in various of the early papers, Hans Pettersson described oceanic heat-flow measurements that he took in 1948; Bullard considered these invalid.)

Bullard, as he said above, returned to England and in 1952 (or 1951?) at the National Physical Laboratory had an instrument built that was "similar to the original one" [*Bullard*,

Article (*cont. on p. 74*)



Scripps Institution of Oceanography

Fig. 1. The heat-probe recorder built in the summer of 1949 by E. C. Bullard and A. E. Maxwell.

Forum

Global Positioning Technology

In an otherwise excellent article ("The Impact of VLBI and GPS on Geodesy," *Eos*, September 27, 1983, p. 569), Dr. John D. Bossler inaccurately reports that the Macrometer, a single channel receiver which came to market only last year and is restricted to relative position use was the first global positioning system (GPS) receiver to become commercially available.

As many in the geophysics community know, Stanford Telecommunications, Inc. (STI), has provided GPS receivers and transmitters commercially for almost 5 years.

STI dual frequency GPS Geodetic Recievers, owned and operated by Shell, NSWC, IBM, and Draper Labs have been used in numerous absolute and relative positioning experiments. In one such experiment, analysis of 5 hours of four satellite phase measurements yielded relative position coordinates on a 28-km baseline which were repeatable to 30 cm in height, 50 cm in E-W direction, and 10 cm in N-S direction.

STI GPS recievers have been used on the North Atlantic aboard the CSS *Hudson* by Norwich, University of New Brunswick, and the Canadian Hydrographic Survey. Here a real time Kalman filter was used to combine GPS ranging and ships motion information to update an inertial navigational system. Further, STI's self-contained model 5010 receivers provide real time positioning with precision on the order of 20 m with no postprocessing.

Paul D. Perreault
Technical Staff
Stanford Telecommunications, Inc.
Santa Clara, CA 95050

Reply

I apologize to Dr. Perreault and Stanford Telecommunications, Inc., for my lack of precision in the subject article. A more proper statement would have been "The first receiver to become commercially available for *geodetic positioning* was the Macrometer Model V-1000 manufactured by Macrometrics, Inc., of Woburn, Massachusetts. We at the National Geodetic Survey are well aware of the important role played by STI in the development of the GPS system.

John D. Bossler
Director of Charting and Geodetic Services
NOAA
Rockville, MD 20852

Biography of Wiechert

For a biography I am writing of Emil Wiechert (1862–1928), professor of geophysics at the University of Göttingen, I would appreciate hearing from any readers who might have material of interest. Photos, letters, and anecdotes about his life are all welcome.

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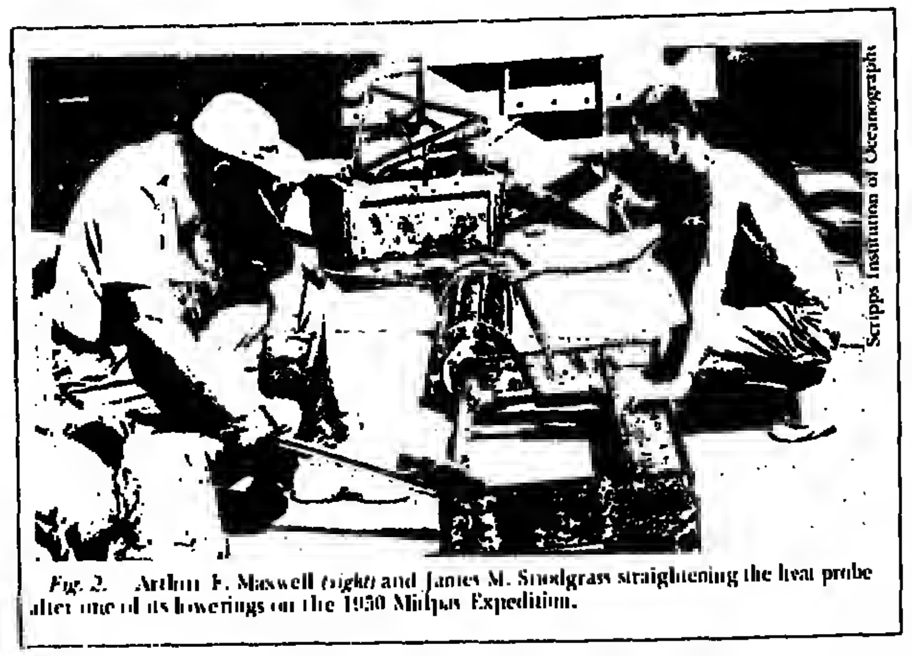

Fig. 2. Arthur E. Maxwell (right) and James M. Snodgrass straightening the heat probe after one of its lowerings on the 1950 Midpac Expedition.

Article *(cont. from p. 73)*

1951). It is difficult to determine from the detailed description of this instrument in that reference what modifications he and his laboratory may have made of the original design. Bullard obtained his first heat-probe measurements with the NPL-built instrument in July 1952 from R.R.S. *Discovery II* in the Atlantic.

Maurice Ewing, at the Lamont (now Lamont-Doherty) Geological Observatory, set out to measure heat flow after using a borrowed Bullard probe in 1953 that failed to work. By 1957 Robert Gerard and Ewing had developed a needle-probe device for measuring heat flow that was attached to the routinely used piston corer [*Gerard et al.*, 1962].

What was so exciting about those early oceanic measurements of heat flow? As *Bullard* [1965] charmingly recounted in the historical introduction to Lee's book, *Terrestrial Heat Flow*, two committees of the British Association for the Advancement of Science, one in 1868 and one in 1935, had been appointed "to consider" underground temperatures and thermal conductivities in mines. Bullard conducted some of the experimental work for the 1935 committee, and, typically for whatever he was doing, he became intrigued by the breadth of the subject. What might be different about the deep ocean?

The state of the art was summarized in the *Bullard et al.* [1956] paper.

... there are only a few dozen reliable measurements of heat flow on the continents. The values range between 0.5 and 3 µcal/cm²sec ... most of them being between 0.8 and 1.4. The mean is about 1.2 µcal/cm²sec.

The continental heat flow is easily accounted for by the radioactivity of the continental rocks. ... Since a large part of the material above the Mohorovicic discontinuity at a depth of about 35 km must be granite or similar rocks, and since some heat must come from below the discontinuity, the difficulty is not so much to find a source for the observed heat as to explain why the flow is not greater than it is. ...

The petrological study of oceanic rocks and seismological work at sea have shown that the crust beneath the ocean basins is strikingly different from that underlying the continents. ...

In view of the striking differences in petrology and structure between the oceans and the continents it might be expected that the heat flowing from the oceans would be only a fraction of that from beneath the continents. In fact this is not so. ...

Since the oceans cover 71% of the earth's surface, a reliable estimate of the amount of heat flowing through them is of great importance in discussions of the earth's thermal history. The oceanic measurements are not, however, much of an addition to the continental ones; in view of the fundamental differences between the oceans and the continents the heat flow at sea poses a problem separate from that of the continental heat flow.

Immediately after the Midpac Expedition, Revelle wrote to Bullard (October 15, 1950; carbon of letter is in archives of the Scripps Institution of Oceanography Expeditions-Midpac):

We managed to get at least seven good temperature sets of temperature gradients in the bottom muds. All but one of these indicate a gradient of around 0.12°C per meter. If your conductivity estimates can be used, this means that the heat flow from the sea floor is the same as that from the continents. To me, at least, this is a very surprising result, but apparently you dyke surprised at all along.

Revelle and Maxwell [1952], in their letter in *Nature*, cited six measurements, with the comment "It is noteworthy that at five of the six localities, covering a total distance of nearly three thousand miles, the computed heat flow lies within 10 per cent of the average value for the continents."

Bullard [1952], in the letter to *Nature*, said that the "communication by Revelle and Maxwell [from the Midpac Expedition] gives a result which is completely unexpected, and demonstrates again how little we know of submarine geology."

When Bullard first concerned himself with measuring the flow of heat through the ocean floor in 1939 and had the opportunity at Scripps Institution in 1949, the tectonic dilemma about differences between land and sea to geologists was sharp. Every bit of evidence was needed to understand the whole earth. As measurements accumulated, patterns of variation in heat flow through the ocean floor began to emerge, and orders of magnitude for "average" values improved. *Langseth and Von Herzen* [1970, p. 299] noted that "a surface flux of up to eight times the earth's average is observed near the axis of the mid-oceanic ridges."

Those early heat-probe measurements, acquired with "a very clumsy apparatus" that began in Bullard's mind and hands, contributed considerably to the global knowledge of today's geophysicists.

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Edward C. Bullard 1907–1980

Professor Sir Edward Bullard was honored by his Queen in 1975 as a "world leader in geophysics." Each of these words is apt:

"world"—because indeed the earth was his domain, in theory and field work, in civilized and remote regions, and at sea;

"leader"—because after listening to and conversing with students and colleagues he nudged them toward carrying out large ideas from half-formed thoughts;

"geophysics"—a field between precise physics and imprecise geology that came into being only because a few outstanding and versatile scientists like Bullard could span the breadth.

"Chance led me into geophysics at a wonderful time," he said, "and it has been among the most rewarding experiences of my life to have played a part in the transformation of a backwater into a bandwagon" [*Bullard*, 1975*b*].

Teddy Bullard carried this off with a unique charm, with a near-Puckish humor, and with an interest in people that made every chat with him memorable and every lecture by him a keen delight. His legacy will prove to be more than his own considerable scientific endeavors: it will include the accomplishments of his students and colleagues and a better effort even by his casual acquaintances. It has been said: "The thing looked so simple when Teddy explained it; it was only later you realized how deeply he understood" [*Matthews*, 1980].

He was an outgoing man, quite lacking in pomposity, yet very aware of status in his acquaintances. He observed the differences between noted erudite comparisons between his homeland and his adopted home country. Although he appeared disinterested in honors, one sensed that they pleased him.

Edward Crisp Bullard was born on September 21, 1907, in Norwich, England, where his father's family were brewers. In his early years he was not an outstanding student, but during high-school years he developed a keen interest in physics through a teacher. He entered Clare College, Cambridge, and graduated with first-class honors in physics. In 1929 he became a graduate student in the Cavendish Laboratory of Lord Rutherford, from which he received his Ph.D. in 1932 with a dissertation on the scattering of slow electrons in gases.

The first position available to the young physicist in those depression years was for the Department of Geodesy and Geophysics at Cambridge, as a demonstrator in surveying. As such, according to Bullard (who wrote a tribute to himself at the request of Walter Munk), he "embarked on a very profitable eight years in which he learned the elements of Earth science and carried out a remarkably diverse series of projects. The success of these was in large part due to the steady support of [Colonel Sir Gerald] Lenox-Conyngham, who was not at all disturbed when told that the police were looking for the perpetrators of an explosion which had left a hole in a road in Leicestershire" [*Munk*, 1978].

The first in the diverse series of projects was measuring small variations in gravity, for which Bullard developed an elegant technique for timing the swings of an invariant pendulum. The work was done during 1933–1934 in the East African Rift Valley, where on one occasion he and his wife Margaret were treed by a lion.

The explosion that disturbed the police was part of a group program to map the Paleozoic layer beneath eastern England by means of seismic refraction. At the 1936 meeting of the International Union of Geodesy and Geophysics in Edinburgh, Bullard met Princeton geologist Richard M. Field. It was Field's insistent theme that geologists must study the ocean floor; *Bullard* [1975*a*] said: "He would not take no for an answer, he would not stop talking, he had no doubts, he was embarrassing and sometimes a nuisance, and yet he struck the match that set earth science alight. ... He invited me to the United States in 1937 ... and sent me out to sea with the Coast and Geodetic Survey and with Maurice Ewing." Upon his return to England, Bullard began seismic studies with Thomas F. Gaskell in the western approaches to the English Channel aboard two Brixham sailing trawlers.

As a third venture into the young field of geophysics, Bullard measured the temperature gradient, or heat flow, on land in England and in South Africa in 1938–1939. This was for a committee of the British Association for the Advancement of Science, appointed in 1935. Work of this kind, said Bullard once, "had been done extraordinarily badly before" [*Shor*, 1984], and elsewhere he noted: "Great difficulty was experienced in finding suitable [boreholes, but] very satisfactory results were obtained from those that were found" [*Bullard*, 1965]. The project led Bullard to pondering a means of measuring heat flow in the deep ocean.

On the advent of World War II, Bullard became an Experimental Officer in the Naval Scientific Service, first to develop methods for protecting ships from magnetic mines, then to develop methods for sweeping acoustic and magnetic mines. He moved on to intelligence investigations pertaining to the German development of rockets and to a study of the most economical ways of attacking firing sites in France. The *Times* noted that Bullard "built up around him an astonishingly versatile and effective establishment which, in the urgency of time, had often necessary, an utter disregard for the formalities of normal civil service rules, and thus moved with a speed impossible for other establishments" [*Matthews*, 1980]. His participation in matters for the Ministry of Defence continued for many years, and he served on the committee for a nuclear-test ban treaty also.

Bullard returned to Cambridge in 1945, where he found the laboratory in a shambles after 6 years of disuse, but he got it into operation and soon became head of the Department of Geophysics. The lack of research funds and lack of access to a research ship frustrated him to the extent that in 1948 he accepted the position as head of the Physics Department at the University of Toronto in Canada. There he initiated theoretical inquiries into the source of the earth's magnetic field, using the early computer system ACE. Bullard's long-pursued work on dynamo theory of the earth's core constitutes his most profound single contribution to knowledge of the earth.

In 1950 Bullard returned to England to succeed Sir Charles Darwin [grandson of the author of *The Origin of Species*] as Director of the National Physical Laboratory at Teddington. While chafing some at the responsibilities, he felt that he was an effective director, and he was able to do a substantial amount of his own researches. In fact, "through his position as director he was able to deploy the entire resources (by no means inconsiderable) of the computing division to carry out extensive numerical work required for the development of his [dynamo] theory" [*Massey*, 1980]. The position and his competence gained him a knighthood in 1953.

Bullard in 1956 resolved his indecision about what kind of life he wanted by returning to Cambridge, again as head of the Department of Geodesy and Geophysics. There he felt that his primary commitment was to his own work and to helping a large number of very able graduate students. It was an era of ferment in the expanding field of geophysics, and Cambridge became a focal point. In 1963, for instance, Harry Hess, J. Tuzo Wilson, Drummond Matthews, and Fred Vine were all at Cambridge, and Bullard was reassembling the continents by computer with deceptive simplicity [*Bullard*, 1964]. Seafloor spreading and plate tectonics were launched, and for those theories Teddy Bullard was a primary catalyst.

In his Cambridge years he also directed a major investigation into electromagnetic induction in the earth; he helped in establishing the technique of determining the age of rocks by the potassium-argon method; and he encouraged the application of modern computer techniques to geophysical problems. Always he was interested in the history of science, and he assembled a considerable library in it. This seemed to be an outgrowth of his interest in people: for an individual's memorial account he wanted to know not only the science but also the minutest details of that person's life. He quite enjoyed writing historical summaries of aspects of geophysics.

Bullard's association with Scripps Institution of Oceanography in La Jolla, California, began with a brief visit in 1948, followed by a 2-month stay in 1949 when his first heat-probe was developed [Shor, 1984]. From the mid-1950's he was a frequent visitor to Scripps, often from 1963 to 1977 he held a part-time appointment as professor of geophysics. When he retired from Cambridge in 1974, he moved to La Jolla permanently. There he continued to work vigorously on what he called "his favorite topics of plate tectonics and the origin of the Earth's magnetic field," and he was drawn into an advisory role to the U.S. government on nuclear-waste disposal. In all, he published some 200 papers (a memorial account and a complete bibliography will be published by the Royal Society of London).

Bullard married Margaret Ellen Thomas in 1931; they had four daughters. Margaret was the author of several novels situated in various places where the family had lived. In 1974, Bullard married Ursula Margery Curnow of New Zealand; she is an accomplished painter and sculptor.

This giant in geophysics died of cancer on April 3, 1980, in La Jolla. Only a few hours before his death he approved final changes on the paper "Direction of the earth's magnetic field in London, 1570–1975," with Stuart Malin of Edinburgh. Teddy made life interesting, and he faced death with courage.

At the change of command in 1976, the Albatross Award of the American Miscellaneous Society was transferred to Edward C. Bullard (right) from Roger Revelle (left). It's a scruffy bird, but a signal honor.

Awards to Edward C. Bullard

Sedgwick Prize, 1936
Fellow, Royal Society of London, 1941
Hughes Medal, Royal Society of London, 1953
Foreign Honorary member, American Academy of Arts and Sciences, 1954
Chree Medal, Physics Society, 1956
Foreign Associate, National Academy of Sciences, 1959
Day Medal, Geological Society of America, 1959
Gold Medal, Royal Astronomical Society, 1965
Agassiz Medal, National Academy of Sciences, 1965
Wollaston Medal, Geological Society of London, 1967
Vetlesen Medal and Prize, Columbia University, 1968
Royal Medal, Royal Society of London, 1975
Bowie Medal, American Geophysical Union, 1975
Albatross Award, American Miscellaneous Society, 1976
Maurice Ewing Medal, American Geophysical Union, 1978

The facilities of the Cambridge Department of Geodesy and Geophysics were named the Bullard Laboratories in 1980.

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This tribute was contributed by Elizabeth N. Shor, *La Jolla, CA 92037.*

EOS

Transactions, American Geophysical Union

The Weekly Newspaper of Geophysics

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Cover. This weak-beam micrograph shows a twist boundary in an experimentally deformed olivine single crystal from San Carlos (Arizona). It was taken with a 125 KeV transmission electron microscope. The scale bar is 2200 nm. Such orthogonal arrays of screw dislocations are also commonly observed in naturally deformed peridotites. They are produced during deformation or annealing events either in the earth's mantle or during the ascent of the olivine-bearing bodies to the surface. The micrograph was taken by Daniel L. Ricoult (advisor: D.L. Kohlstedt), Department of Materials Science and Engineering, Cornell University, Ithaca, New York.

An Invitation

Would you like to be on the cover of *Eos*? If you have any illustrations with both aesthetic charm and scientific interest—photographs (preferably black and white) of geophysical phenomena, experimental results, or graphs—*Eos* would like to consider them for publication on the cover. Send the original illustration or 8 x 10 inch (20 x 25 cm) glossy photo with a short (50–200 words) explanation that can serve as a caption. You may also submit a more extensive news item or even a short article to accompany a proposed cover. Captions will be by-lined. If the material has been previously published, please supply a copyright release from the copyright owner. Send it to *Eos* Cover, AGU, 2000 Florida Avenue, N.W., Washington, DC 20009.

News

Cretaceous Boundary

Four years after it was first proposed, the theory that a colossal meteorite struck the earth 65 million years ago continues to build toward a consensus—this despite recent findings that volcanic eruptions might also have caused the "iridium anomaly" that is the impact theory's best evidence (*Eos*, February 7, 1984, p. 41).

High concentrations of iridium at the Cretaceous-Tertiary boundary were first noticed in sediments from Italy, Denmark, Spain, and other locations around the world by Walter Alvarez, his father Luis, and their Berkeley colleagues. They concluded that the source of the extra iridium must have been extra-terrestrial because the element shows up in crustal rocks only in very small amounts and no one could think of a mechanism that would distribute iridium from the mantle so widely around the surface of the globe. The Alvarez group postulated that a meteorite about 10-km wide had collided with the earth, throwing up a planet-encircling cloud that blocked out sunlight, ended photosynthesis, and snuffed out many of the land and sea creatures of the Mesozoic, including the dinosaurs.

Until recently, the iridium anomaly was virtually the only support for this idea. In November, however, two researchers from Yale University, Jean-Marc Luck and Karl Turekian, reported what may be even more compelling evidence. They analyzed osmium isotopes in boundary-layer rocks from Denmark and Colorado—osmium is another platinum-group metal that shows up anomalously high in these sediments—and found nearly equal amounts of ^{187}Os and ^{186}Os, an isotopic ratio much more characteristic of meteorites than of crustal rocks. Luck and Turekian had first hypothesized that if a normal geochemical process such as precipitation from seawater had concentrated the osmium, they would see the higher ratio found in terrestrial rocks. After examining a number of oceanic manganese nodules, they found that this was indeed the case, and so concluded that it was a meteorite impact (or possibly two impacts, since they found different ratios at the two sites) that deposited the osmium.

With this new information, is there now a consensus on the meteorite theory? Not yet, says Charles Orth of the Los Alamos National Laboratory, who provided Luck and Turekian with the sample from Colorado's Raton Basin and who was the first to find the iridium anomaly in freshwater sediments. "We can't yet lay to rest a volcanic source—we need to learn more about the platinum metal ratios in the mantle." Recent findings at Kilauea in Hawaii show that iridium can be concentrated on the surface after settling out from volcanic emissions. It would have required an enormous explosion to distribute this volcanic material globally in the Cretaceous-Tertiary boundary layer, however, and the sort of slow, oozing flows that normally accompany eruptions like Kilauea are not consistent with this.

Most frustrating of all for the meteorite-theory faithful is the continuing lack of any evidence of an impact. So far, about 100 craters around the world have been evaluated as possible candidates, according to Thomas J. Ahrens of the California Insitute of Technology, "but none seem to be the right size and age." For this reason, many people favored the idea of an ocean impact right from the start. After all, Ahrens points out, the climate was warmer 65 million years ago, water wasn't locked up in polar ice caps, and there was more ocean.

An ocean impact would also explain why no crater has been identified—more than half the ocean floor that existed 65 million years ago is gone, subducted back into the mantle. Furthermore, recent reports of small spheroids that appear to be altered "impact droplets" of basaltic origin (found in boundary layer clays in northern Italy and the central Pacific by Alessandro Montanari and colleagues at Berkeley) make the case for a seafloor impact even stronger. But where are the remnants of the great tsunamis and submarine landslides that would have followed such an event? No one has yet come up with a good answer.

Meanwhile, Charles Orth and others are looking for iridium anomalies at other periods in geologic history. This leads to another question: If meteorites do occasionally hit the earth, are they responsible for mass extinctions? Orth and various coinvestigators have checked for high iridium concentrations in clays known to be deposited during other periods of wholesale extinctions, including two trilobite-brachiopod boundaries in the late Cambrian, where they found no iridium anomaly. Orth has also checked boundary layers from the Ordovician-Silurian and the late Devonian. No anomalies there, either. But he continues to search for other examples of iridium excess across time and plans to widen the geographic range of his studies of the Cretaceous-Tertiary boundary layer to establish a more global pattern.

Walter Alvarez, with whom it all started, will have a paper in *Science* next month in which he counters the counterargument that the extinctions at the end of the Mesozoic were gradual and so couldn't have been caused by one catastrophic event. Alvarez and his coauthors, based on a review of the existing paleontological literature, see "a sharp dropoff" right at the iridium-rich boundary layer for four groups of marine invertebrates: ammonites, one group of bryozoans, brachiopods, and bivalves. "Some paleontologists have said that the dropoffs were more gradual," Alvarez told *Eos*, "but we looked back at the record, rather than what they say the record shows."—*TR*

Planetary Science Budget

The fiscal year 1985 budget for the solar system exploration program of the National Aeronautics and Space Administration (NASA) is very promising (n/5Eos, February 14, 1984, p. 49). As announced by the Office of Management and Budget, the document includes a new start-up program for the MGCO (Mars Geoscience Climatology Orbiter) mission. Mixed in with the set of encouraging figures of NASA's Space Science and Applications budget plan, unfortunately, is the more uncertain area of funding that includes support for the Research and Analysis program. These programs seem to be less visible in the budget, but they include important components of the space science program. A large portion of these funds is provided to university scientists to support research at the highest levels of excellence.

In a recent letter to members of the academic scientific community, William L. Quaide, chief scientist of NASA's Solar System Exploration Division, expressed a note of uncertainty and caution about the fiscal year 1985 budget operating level for the Research and Analysis programs. According to Quaide: "The reduction in [Research and Analysis] funds will require that we abandon, temporarily, plans to address all the problems that are facing us, and concentrate our support on a few specific areas to preserve some continuity of participation and to maintain capabilities for future missions."

Quaide pointed out that an augmentation of $15 million will be needed to maintain the support level of funding realized by the fiscal 1984 appropriation. Indeed, an augmentation for the research and analysis program is included in the president's budget, but only in the amount of $6 million. Conceivably that number could be increased by Congress if the need were made known.—*PMB*

Titan Ocean: Ethane, Methane, Nitrogen

Detection of the atmosphere of Saturn's satellite Titan by the Voyager I spacecraft indicated an abundance of only 3 mol % methane (CH_4). Recently J. I. Lunine, D. J. Stevenson, and Y. L. Yung calculated that 3 mol % methane is sufficiently low to preclude the stable coexistence of liquid methane on Titan's surface, which has a temperature of 94 K (*Science, 222*, 1229, 1983). Instead, Lunine et al. suggest that Titan's atmospheric methane may have broken down by a catalyzed photochemical reaction to ethane (C_2H_6). The resulting ocean would consist of a mixture of C_2H_6 and CH_4 in the proportion of 3 to 1.

The dissociation steps of C_2H_6 involve loss of hydrogen by escape, making the process irreversible. The postulated set reactions are: $2CH_4 \rightarrow C_2H_6 + H_2$ and $2CH_4 \rightarrow C_2H_6 + 2H$. The intermediate molecule C_2H_2 plays the role of catalysis and shielding of C_2H_6 from photolysis. CH_4 is calculated as being broken down in Titan's atmosphere at a rate of 1.5×10^{10} cm^{-2} s^{-1}. The evolved H and H_2 would leave the exosphere at rates of 5.5×10^9 and 7.2×10^9 cm^{-2} S^{-1} respectively. The result is the production of an ethane-rich ocean of 1 km depth. The composition of Titan's present ocean was deduced from calculations of the C_2H_6–CH_4, N_2–CH_4, and N_2–C_2H_6 binary systems. To be consistent with Voyager observations, it was noted that the existence of several percent propane in the ocean would be compatible with the calculated proportions. The calculations also include a 100–200 km thick layer of solid C_2H_2 on Titan's ocean floor.

What is lacking to make a case for an ethane-rich ocean is more observational data. Lunine et al. note that important tests of the model are verification of the existence of CH_4 clouds with the correct opacity, detection of C_2H_6 saturation of the lower troposphere, and radar evidence of an ocean.—*PMB*

Alaskan Transect

A cooperative geophysical/geologic transect of the Alaskan crust and upper mantle is being organized by the U.S. Geological Survey (USGS), the Alaska Division of Geological and Geophysical Surveys (ADGGS), the University of Alaska, and Rice University. The project is to be known as the Trans-Alaska Lithosphere Investigation (TALI). The route of TALI lies along the north-south corridor of the trans-Alaska oil pipeline between Prudhoe Bay and Valdez and extends offshore across the Pacific and Arctic continental margins. The transect will incorporate several supplementary profiles intersecting the primary route. TALI is envisaged as a coordinated multidisciplinary effort among government, academic, and industry scientists and institutions.

To prepare a prospectus for the transect, a workshop will be held in Anchorage on May 29 prior to the meeting of the Seismological Society of America and the Cordilleran Section of the Geological Society of America (May 30–June 1). The National Science Foundation is co-sponsoring the workshop.

Some of the studies that will constitute important elements of TALI are under way or will be initiated this year. Rice University and the University of Alaska are engaged in a cooperative study of the kinematics of deformation in the Brooks Range. ADGGS and USGS will continue geologic mapping and investigations in various areas along or near the transect route. To help launch TALI, the USGS will start this summer its Trans-Alaska Crustal Transect (TACT) project to investigate the structure and evolution of the crust, using seismic refraction/reflection, geologic, gravity and magnetic techniques. The TACT project will begin along the southern onshore segment of the transect, between Valdez and the Alaska Range; several investigators from other institutions will be directly involved. Other institutions, including Cornell University (COCORP), Lamont-Doherty Geological Observatory, and the University of Utah are exploring participation in TALI in 1985 and later years. The goal is to complete the transect by the end of this decade.

Geologists and geophysicists interested in participating in TALI or the May workshop should contact: Robert Page, U.S. Geological Survey, Mail Stop 77, 345 Middlefield Rd., Menlo Park, CA 94025 (415-323-8111); or John Davies, Alaska Division of Geological and Geophysical Surveys (907-474-6166), or David Stone, University of Alaska (907-474-7622), both at the Geophysical Institute, University of Alaska, Fairbanks, AK 99701.

This news item was contributed by Robert A. Page, *who is with the U.S. Geological Survey, Menlo Park, CA 94025*

Geophysicists

Two AGU members received Fulbright scholarships for 1983–1984: ***John R. Holloway***, professor of chemistry at Arizona State University, will be working in Australia. ***Teh-Lung Ku***, professor of geological sciences at the University of Southern California, will be working in France.

In Memoriam

The following AGU members are recently deceased. Their AGU section affiliation and year of joining AGU are shown:

Jaime Amorocho, 64. An AGU Fellow, Hydrology, joined 1952.

Grover B. Crisp, 43, on April 20, 1983. Geodesy, joined 1980.

Mahdi Salih Hantush, 63, on January 14, 1984. Hydrology, joined 1949.

Allen A. Jergins, 80. Seismology, joined 1943.

Jack H. Meek, 66, on October 12, 1983. Solar-Planetary Relationships, joined 1959.

Heinz G. Poetzschke, 68, in November 1983. Geodesy, joined 1976.

Books

The Boundary Integral Equation Method for Porous Media Flow

James A. Liggett and Philip L.-F. Liu, Allen and Unwin, New York, 1983, xi + 255 pp. $35.

Reviewed by Mary P. Anderson

Just as groundwater hydrologists are breathing sighs of relief after the exertions of learning the finite element method, a new technique has reared its nodes—the boundary integral equation method (BIEM) or the boundary equation method (BEM), as it is sometimes called. As Liggett and Liu put it in the preface to *The Boundary Integral Equation Method for Porous Media Flow*, "Lately, the Boundary Integral Equation Method (BIEM) has emerged as a contender in the computation Derby." In fact, in July 1984, the fifth International Conference on Boundary Element Methods in Engineering will be held aboard the *Queen Elizabeth II*, en route from Southampton to New York. These conferences are sponsored by the Department of Civil Engineering at Southampton College (UK), whose members are proponents of BIEM. The conferences have featured papers on applications of BIEM to all aspects of engineering, including flow through porous media. Published proceedings are available, as are textbooks on application of BIEM to engineering problems. There is even a 10-minute film on the subject.

The applications of BIEM to flow through porous media are explored by the other major proponents of this method in this textbook by Liggett and Liu of Cornell University. Briefly, BIEM involves an application of Green's formula, which allows the dimensionality of a problem to be reduced. The method consists of placing nodes on the boundaries of the domain—a line for 2-dimensional problems, an area for 3-dimensional problems—and integrating over the boundary. Then application of Green's formula makes it possible to solve for the head at any point in the interior of the domain. The catch is that it is necessary to know both heads and their normal derivatives along the boundary. Fortunately it is possible to derive an expression relating head along the boundary to its normal derivative, so that given either head or its derivative, it will be possible to solve for the other at each node. An advantage of the method is that any approximation involved in discretization is limited to the boundary, although there is a boundary layer effect causing heads near the boundary to be more subject to numerical errors than heads in the interior of the domain. Liggett and Liu suggest that the thickness of this boundary layer is approximately equal to the length of a boundary element.

The book is written at an advanced level and requires a thorough familiarity with calculus as well as the theory of flow through porous media and finite element techniques. The examples used to illustrate the use of the BIEM reflect the training of the authors, in that classical engineering problems in flow through porous media are emphasized including flow through dams, flow around a cut-off wall, flow to wells, seepage from a pond, and recharge from circular and rectangular regions.

A mathematically compact introductory chapter reviews the basic theory of flow through porous media and the relevant governing equations and serves to introduce the reader to the authors' notation. Chapter 2 describes the application of the method to simple problems involving Laplace's equation and linear interpolation of head between boundary nodes (linear boundary elements). Chapter 3 deals with more sophisticated interpolation schemes, known as special elements. Chapter 3 also introduces a powerful feature of the method, whereby infinite domains can be treated by using an analytical solution to construct a special element to extend the problem domain from the near-field solution, where a detailed solution is desired, out to infinity. In this way the problem of defining nearfield boundary conditions is avoided.

BIEM treats heterogeneity by dividing the problem domain into several homogeneous zones. The concept of zones and the equation manipulation (by stretching the coordinate system) required to treat anisotropy are discussed in Chapter 4. Chapter 5 describes the solution of transient free-surface problems using steady state governing equations and transient boundary conditions on the free surface. Chapter 6 deals with axial symmetric flow problems, including recharge from circular areas and flow to wells. Chapter 7 deals with 3-dimensional solutions of Laplace's equation applied to well problems and to unconfined aquifers using a transient boundary condition on the free surface. Chapter 8 discusses the application of BIEM to problems involving a sharp interface between two fluids, e.g., salt water intrusion without dispersion. Chapter 9 focuses on the intriguing way in which boundary elements can be combined with standard finite elements. It is clear that a finite element approximation is more appropriate for heterogeneous problems. However, if only a portion of the problem domain is heterogeneous, it may be advantageous to use boundary elements for the nonhomogeneous portion. Examples presented in chapter 9 include unsaturated-saturated problems, in which finite elements are used for the unsaturated zone and boundary elements are used for a homogeneous saturated zone. Throughout chapters 2–9, the results of the BIEM are compared to analytical solutions and to results of finite difference and finite element solutions drawn from the literature.

Chapter 10, the longest in the book, discusses the application of BIEM to problems involving regional groundwater basins including leaky aquifers. The problems are handled using a quasi 3-dimensional approach, whereby flow through aquifers is assumed to be horizontal and flow through confining beds is assumed to be vertical. Reference is made to a computer code called GM5, which was used to simulate the Yun-Lin aquifer in Taiwan. Output from the simulation is presented but the code is not, owing to space limitations. However, sample FORTRAN codes are presented in chapter 11. The code GM8 solves the Laplace equation using linear elements; GM9 is an extension of GM8 that allows for the use of special elements; and the program DAM solves transient free surface flow through a dam of constant hydraulic conductivity. In addition to the codes, documentation of input as well as sample input and output data sets are presented. An appendix containing a list of symbols used in the book and discussion of some of the finer mathematical points is also provided.

The authors succeed in presenting a convincing case for the utility of BIEM for certain types of engineering problems in flow through porous media. Yet the book's bibliography gives only a few references on applications of BIEM to flow through porous media and these are largely papers by Liggett or Liu. Hence, one is left wondering why the method seemingly has not caught on among specialists. The arsenal of mathematics needed to understand and program BIEM rivals the finite element method, and as a result BIEM is unlikely to take hold among "the masses" until the documentation and release of general "canned" programs similar to the one discussed in chapter 10. Still, the book is a good start in bringing BIEM to the attention of groundwater hydrologists and will be especially useful to those who already have a working knowledge of finite elements and who have occasion to solve those classes of problems for which BIEM is particularly suited.

Mary P. Anderson is with the Department of Geology and Geophysics at the University of Wisconsin-Madison, Madison, WI 53706.

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AGU Congressional Science Fellow: Midterm Report

Jack Fellows

As the recipient of the 1983–1984 AGU Congressional Science Fellowship [*Eos*, September 6, 1983, p. 543] I have been asked to report on my midterm experiences. I have learned two major aspects of business on Capitol Hill. First, I am impressed by the tremendously important relationship between science and the political world of Congress. Congressmen need and actively seek technical advice on legislative issues. As a scientist, one's point of view can have an impact on society as well as on one's discipline.

Second, I have been overwhelmed by the dedication of members of Congress and their staff. As in corporations and academia, the politics and bureaucracy can suffocate many a good idea. However, considering these things, plus overlapping committee jurisdictions and the inherently difficult legislative process, it is still a wonderment to me that Congress manages to produce anything.

The AGU Congressional Science Fellowship consists of four phases: selection, orientation, placement, and assignment. I would like to discuss each one of these phases; through each, I feel that I have learned and grown both personally and professionally.

Selection

Application required a letter of intent, vita, and letters of reference. Once short-listed, one wrote and presented a congressional briefing to a peer review board. I chose deep-seabed mining as my topic; it offered me the opportunity to show my ability to research a briefing on (to me) a totally foreign topic.

Orientation

In addition to AGU, 19 professional societies enroll their Fellows in the AAAS Congressional Orientation Program. These societies include the AGU, ACS, APhA, NSPE, IEEE, and others. This year there were 42 Fellows, ranging in age from 28 to 50. All but 10 of the Fellows have their doctorate.

Fellows have been placed in Congress, OTA, and the State Department. Besides constantly tapping our Fellows network (the Fellowship) for legislation information, we have regular dinner and luncheon seminars and informal meetings. We set up our own seminars, which have so far included topics like Central America, ethics, the "gender gap," arms control, risk assessment, and lobbying. I have had the great pleasure of being the chairman of this year's Fellowship.

The goal of orientation is to prepare one for placement in the personal office of a member, on a committee staff, or in some other congressional branch. This program is a rather grueling sequence of congressional briefings from congressional agencies, members of Congress, committee staffs, historians, AAAS representatives, former Fellows, and others. In addition, there are meals and receptions with speakers, members of Congress, and congressional staff. The Fellows' friendship develops through this exhausting but thoroughly enjoyable "boot camp." The AAAS orientation is similar to the orientation that freshman congressmen undergo. By the end of the 2-week period, one has a better understanding of how Congress is structured, how it operates, and what forces both emanate from Congress and shape Congress. The contact with congressmen and staff makes one aware of the important "players" in Congress. Now was the time for placement.

Interviews

We were now let loose on Capitol Hill to find a position for a year. All the interesting aspects of congressional work we had heard about during the orientation made it very difficult to chose the appropriate member, committee, subcommittee, or congressional branch. Prior to the orientation, I had chosen to work on hydrologically related issues. However, during orientation I had added to my list the areas of innovation, productivity, industrial policy, high-tech development, and foreign affairs.

AAAS provided us with a room full of phones in the Senate Hart Office Building, a list of congressmen looking for Fellows, and interview comments from last year's Fellows. The AAAS leadership and support are indispensable to the success of this program.

I left my condensed, one-page resume at more than 30 offices. I eventually interviewed with 13 personal offices and 6 committees on both the Senate and House sides. I went back to many of these offices two or three times. There was a constant exchange of interview information through the Fellowship network and relatively little competition. We found out a lot about ourselves, Congress, legislative issues, and especially the underground tunnels interconnecting the congressional office buildings.

In spite of all my criteria for choosing an office in which to work, my choice rested very heavily upon office "chemistry." I chose to work in the personal office of Rep. George E. Brown, Jr. (D-Cal.).

Work

Besides being on the House Science and Technology Committee and the House Agricultural Committee, Rep. Brown has a personal interest in remote-sensing applications, water resources, and data base management, all of which I was involved in at the University of Maryland. Some of the other Fellows have told me that the members they work for do not even know their Fellow's name. I felt quite fortunate in this respect, because I get a chance to talk with Rep. Brown frequently and I have accompanied him on hearings and speech trips.

For the first 2 months, I read congressional reports, attended meetings, hearings, and floor debates and familiarized myself with

AGU (cont. on p. 78)

AGU *(cont. from p. 77)*

congressional and office procedures. I was given industrial policy, space, information, and water resources policy as issue areas. I was not given a specific project, except a speech on the role of the federal government in science and technology. During this time I helped on office projects and constituent work. (The volume of letters, requests, and information coming in to a congressional office is staggering.) I attended Congressional Research Service programs to familiarize myself with the Library of Congress and its data bases.

In November, Rep. Brown asked me to examine the possibility of drafting legislation for the commercialization of Landsat based on discipline resolutions and not the 80-m resolution of current proposals. I learned a great deal about possible suggestions at the National Telecommunication Conference and Jet Propulsion Laboratory.

However, after Congress adjourned in late November, I was asked to write four speeches on industrial policy, innovation, R&D, and productivity topics. The Landsat proposal was shelved for a while.

During the recess Rep. Brown also asked me to examine an Office of Technology Assessment recommendation that the United States needed a National Center for Water Resources Research modeled after the successful National Center for Atmospheric Research. I have made some proposals for such a structure that have generated interest with Rep. Brown, members of the Interior Committee, and the National Association of State Universities and Land Grant Colleges. The timing may actually be correct for this legislation. A lot of politics and arrangements must be worked out before such a bill will be introduced. However, this legislation has now become my main focus. The chances are small that it will make it to hearings before I leave, but I am endeavoring to lay the foundation.

I am enjoying the Hill very much. I know already it has had a great impact on my life. I know I will remain politically active when I return to the scientific community, and I would highly recommend this program to anyone regardless of profession or age.

AGU Membership Applications

Applications for membership have been received from the following individuals. The letter after the name denotes the proposed primary section affiliation.

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Meetings

Announcements

Elevation Datums

Elevation datums are the subject of a 1-day program on March 13, 1984, at the annual meeting of the American Congress on Surveying and Mapping (ACSM) and the American Society of Photogrammetry in Washington, D.C. Jointly sponsored by ACSM, the American Association for Geodetic Surveying, and AGU's Geodesy Section, the session will feature 11 papers. Registration for the session at member rates will apply to AGU members. For further information, contact Thomas J. Lauterborn, Rte. 1, Box 127, Waterford, VA 22190, telephone 703-882-3334.

The paper titles and authors for the program are as follows:

Historical Overview of Vertical Datums, *Herbert W. Stoughton*
Geometrical Aspects of Vertical Datums, *J. D. Bool, F. W. Young, and R. Mozacchi*
Defining Vertical Datums: What Is Zero?, *Richard H. Rapp*
Aspects of a New Height System for North America, *Sandford R. Holdahl*
Fennoscandia Uplift, Plate Tectonics, and Seismicity, and Their Effects on Vertical Datums, *Larry Brown*
Definition: Oceanographic Approach, *Bruce C. Douglas and Robert E. Cheney*
The Determination of Geodetic Height From Laser Tracking Data, *David E. Smith and Demos G. Christodoulidis*
Deep Sea Techniques and Vertical Datums, *William E. Carter*
Gravimetric Approach in Defining Vertical Datums, *Lennon E. Wilcox*
NAVD 1988: A Status Report, *Charles T. Whalen*
The Administrative, Political, and Economic Aspects to Be Considered in the New Definition of the Vertical Datum, *John D. Bossler*

Geophysical Year

New Listings

June 11–12, 1984 **Fifth European Conference on Environmental Pollution,** Amsterdam. (V. M. Bhatnager, Box 1779, Cornwall, Ontario K6H 5V7.)

July 5–6, 1984 **Second Symposium on Plasma Double Layers and Related Topics,** Innsbruck, Austria. (R. Schrittwieser, Inst. for Theoretical Physics, University of Innsbruck, Sillgasse 8, A-6020 Innsbruck, Austria.) (The deadline for registering is May 1.)

September 20–21, 1984 **International Symposium on Environmental Pollution,** Site To Be Announced. (V. M. Bhatnager, Box 1779, Cornwall, Ontario K6H 5V7.)

September 24–26, 1984 **International Water Well Exposition,** Las Vegas, Nevada. Sponsor, National Water Well Association. (National Water Well Association, 500 W. Wilson Bridge Rd., Worthington, OH 43085; tel.: 614-846-9355.)

October 10–12, 1984 **Seismological Society of America Eastern Section 56th Annual Meeting,** St. Louis, Mo. (Robert B. Herrmann, Department of Earth and Atmospheric Sciences, St. Louis University, P.O. Box 8099, St. Louis, MO 63156; tel.: 314-658-3120.) (Abstract deadline is September 4.)

November 1984 **Mexican Geophysical Union Annual Meeting,** La Paz, Baja California Sur, Mexico. (Union Geofisica Mexicana, A.C., Comite Organizador Reunion 1984, Apartado Postal 1805, Ensenada 22800, B.C.N. Mexico.) (Abstract deadline is August 1.)

April 9–11, 1985 **Fifth Annual Front Range Branch Hydrology Days,** Fort Collins, Colo. (H. J. Morel-Seytoux, Dept. of Civil Engineering, Colo. St. Univ., Fort Collins, CO 80523; tel.: 303-491-5448 or 8549.)

June 26–28, 1985 **U.S. Symposium on Rock Mechanics,** Rapid City, S. D. Sponsor, South Dakota School of Mines and Technology. (Eileen Ashworth, Chairman, 26th U.S. Symposium on Rock Mechanics, Department of Mining Engineering, South Dakota School of Mines and Technology, Rapid City, SD 57701–3995; tel.: 605-394-2344.)

August 19–30, 1985 **23rd General Assembly of IASPEI,** Tokyo, Japan. (Ryosuke Sato, Secretary-General of the 23rd General Assembly of IASPEI, c/o Inter Group Corp., Akasaka Yamakatsu Bldg., 8-5-32, Akasaka, Minato-ku, Tokyo 107, Japan; tel.: Tokyo (03) 479-5311.)

The Geophysical Year calendar last appeared in the December 6, 1983, issue.

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